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A CRITICISM OF THE DARWINIAN THEORY. *The Mutation Theory. Experiments and Observations on the Origin of Species in the Vegetable Kingdom.* By Prof. H. de Vries. Translated by Prof. J. B. Farmer and A. D. Darbshire. Vol. ii., *The Origin of Varieties by Mutation.* Pp. viii+683. (London: Kegan Paul and Co., Ltd., 1911.) Price 18s. net.

THE first volume of Prof. de Vries's great work was devoted in the main to an account of the results of the experimental cultivation of an *Oenothera* of uncertain origin, and of the conclusions he deduced from them. The second volume is more general in its scope, and is in effect a criticism of the Darwinian theory. The two might conveniently have been published in the reverse order. That, at any rate, will be found the best way to read them to get a clear idea of the author's point of view.

It is remarkable that pangenesis, the part of Darwin's theory which was received with least enthusiasm at the time of its publication, now almost overshadows all the rest. As Strasburger remarks:—

"Darwin's idea that invisible gemmules are the carriers of hereditary characters, and that they multiply by division, has been removed from the position of a provisional hypothesis to that of a well-founded theory."

De Vries thinks that the phenomena of variability and mutation follow deductively. And for this he claims the support of Darwin himself, who points out ("Variation," ii., 396), "on the hypothesis of pangenesis that variability depends on at least two distinct groups of causes":—(i) when the gemmules are unmodified, but deficient or superabundant, or are rearranged, or become active after being dormant; (ii) when they are modified through changed conditions. The former, Darwin thought, explained "much fluctuating variability"; the latter, the appearance of "new and changed structures." It is worth notice that Darwin uses here the word "mutation," but only apparently in the algebraic sense of permutations of the gemmules.

It is interesting to compare with Darwin's conclusion the form in which de Vries restates it in terms of his own views. I have inserted for clearness the corresponding numbers:—

"(i) Numerical changes of the pangenes are . . . the basis of fluctuating variability. Changes of the position of the pogene in the nucleus lead to the retrogressive and degressive mutations; (ii) whilst to account for progressive mutation we must assume the formation of new types of pangenes" (p. 645).

To de Vries, "fluctuating variability is a function of nutrition." It is also "linear, and oscillates only in a plus and minus direction." Mutation, on the other hand, "necessarily assumes a variability in all directions." It seems abundantly clear that "fluctuating variability" has by no means the same meaning to Darwin and to de Vries. It must be explained that in retrogressive mutation, characters become

latent; in degressive, latent characters become active (p. 72).¹ De Vries states (p. 56) that

"one of the chief objects of the book is to show that ordinary or fluctuating variability does not provide material for the origin of new species."

But if it is merely a case of an individual organism being a little better or a little less nourished, I do not know that I should disagree. Darwin, however, would certainly not have agreed that ordinary variability was linear, or that, seeing he traced its cause to "changes of any kind in the conditions of life," that it was dependent exclusively on nutrition. The conditions throughout a population can never be so uniform that some amount of fluctuating variability is not excited. But if they are constant on the average, there is no resultant selective action. Thus the flora of Egypt has remained stable since at least the date of the Trojan war.

De Vries is extremely anxious to make Darwin responsible for the mutation theory. "It was Darwin who first attempted in various cases to distinguish between" mutability and variability. It is quite true that Darwin distinguished between the multiplication or reshuffling of his pangenes and their modification. But there is no warrant for asserting that he thought the former played no part in the origin of species. He assumed that "variability of every kind is directly or indirectly caused by changed conditions of life" ("Variation," ii., 255). Without variation, "natural selection can do nothing." All that he conceded was that variation might have a considerable range. But he was far from rejecting the efficiency of fluctuating variability. "Under the term 'variations' it must never be forgotten that mere individual differences are included" ("Origin," sixth edition, p. 64); "natural selection is daily and hourly scrutinising, throughout the world, the slightest variations" (l.c., p. 65).

Another misconception, for which, however, de Vries is not responsible, is that Darwin's theory of pangenesis ultimately led him to a modified acceptance of Lamarckism, i.e. that new structures might arise in direct response to the environment. De Vries himself would not accept this. In Darwin's view, the modified pangenes would simply supply material for more ample variation, but natural selection would still determine the result.

De Vries is convinced, nevertheless, that Darwin was at heart a mutationist, and that his belief in discontinuous variability "under Wallace's influence gradually shifted into the background." There seems here to be a double misconception, which it is not difficult to clear up. Darwin, writing to Wallace in 1869, said:—

"I always thought individual differences more important than single variations, but now I have come to the conclusion that they are of paramount importance, and in this I believe I agree with you" ("Life and Letters," iii., p. 107).

It is clear that by individual differences he means fluctuating variability. He was preparing at the time

¹ This seems explicitly stated; I cannot reconcile it, however, with the equally explicit statement on p. 576, where the meanings of progressive and degressive appear to be interchanged.

the fifth edition of the "Origin," and in this there appears a passage of which it is sufficient to quote the first sentence:—

"It may be doubted whether sudden and great deviations of structure, such as we occasionally see in our domestic productions, more especially with plants, are ever permanently propagated in a state of nature" (p. 49).

But, as explained on p. 104, it was Fleeming Jenkin, and not Wallace, who led him to minimise the importance of "single variations, whether slight or strongly marked"; and as these are mutations, Fleeming Jenkin's argument cuts to the root of de Vries's theory.

But the real divergence between Darwin and de Vries is not so much as regards variability, but selection, the importance of which the latter consistently minimises. To Darwin, variability of any kind merely supplied the field in which selection works, and "sinks to quite a subordinate position in comparison." With de Vries it is exactly the opposite. "Specific characters do not arise by selection." The combinations of characters which arise from sexual reproduction are, however, subject to it. With respect to adaptations, which is the crucial point, de Vries speaks with a more uncertain voice than in his "Plant Breeding." "Everything points to the conclusion" that mutation "will explain adaptations just as completely, or, rather, just as incompletely," as natural selection acting on fluctuating variability. This is a cryptic utterance at best. But if natural selection cannot produce specific characters, it cannot endow them, except at haphazard, with survival-value; in that case, its inability to account for adaptations seems a necessary consequence. He finally sums up his position:—

"I willingly admit that almost anything can be squared with the theory in a very plausible way; . . . but this is not science."

Some of us think that it is, and there we must leave it.

De Vries has devoted immense labour to the investigation of the cases, not infrequent, where seedlings occur with more than two cotyledons. Two is a reduced whorl, and a whorl may be explained as due to the suppression of internodes. The number of leaves in a whorl is often variable, though seldom so in the case of opposite leaves; this is in accordance with the principles of the "repetition of similar parts." That two cotyledons should be the rule may well be adaptive, seeing that they have to be packed in the seed. That there should occasionally be more is a not improbable mutation. It is interesting to note that de Vries entirely failed to fix tricotyly by selection. Nor is tricotyly, although it appears to be not uncommon, often followed by trimerous leaves. De Vries has been more fortunate than I have been with the sycamore, as he raised two high trees with "branches in trimerous whorls." He says nothing about the leaves. I signally failed in raising a number of tricotyledonous seedlings; in the third year they all reverted to the opposite-leaved arrangement. And I have only come across a single case of the wild maple with trimerous leaves. This serves as an

illustration, if one were wanted, of how little survival-value mutations possess in nature.

There can be no doubt that what we want is a purely empirical study of the variation under artificial but precise conditions of some clean-cut species free from any suspicion of hybrid origin. It would have to be done on a considerable scale, and as it would have to extend over a long period of years it would be a tedious and laborious business. I have some hopes that it may be undertaken in America. The horticultural papers are now full of what is taking place with *Primula obconica*, which from a condition of stability has passed into one of high variability. But, as de Vries very justly remarks, the available records in such cases "lack precision." But he arrives at the interesting conclusion that

"in horticulture . . . mutations are largely of the retrogressive or degressive kind. Discontinuous formation of species on the progressive line is much rarer" (p. 602).

Here species is used in the de Vriesian, not the Linnean, sense. The meaning, I take it, is that latent characters become active or the reverse. On an earlier page (p. 4) he seems to imply that all the cultivator can do is to evoke latent characters. "The first condition necessary for raising a novelty is to possess it."

Some fifteen years ago I made a careful study of what could be ascertained as to the cultural evolution of *Cyclamen latifolium*. I may state the conclusion, which I confess I was not prepared for:—

"The general tendency of a plant varying freely under artificial conditions seems to be atavistic, i.e. to shed adaptive modifications which have ceased to be useful, and either to revert to a more generalised type or to reproduce 'characters which are already general in other members of the same group.'"

As might have been expected, this had not escaped Darwin, from whom ("Origin," sixth edition., p. 127) I had quoted the concluding words of the sentence.

De Vries still maintains the singular distinction which he draws between cultural variation in horticulture and in agriculture; perhaps on the Continent the two arts are more distinct than with us, where it can hardly be maintained. "Horticultural varieties are generally constant" (p. 76). Agricultural races "remain dependent on continued selection, and do not really become constant" (p. 422).

The Darwinian theory rests on a number of converging lines of argument, and derives its probability from their cumulative force; just as in a law court each branch of evidence may be slender in itself, yet the conclusion to which they all point has a higher degree of probability than any one taken separately. De Vries's attack on natural selection, I must confess, would not shake my faith even if I found it more convincing than I do. But he is entitled to the merit, which he justly claims for himself, of having probed variability by a rigorous experimental method. It is much to be wished, but scarcely to be hoped, that others will follow him in his lifelong devotion to so laborious a research.

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